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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Response of stratospheric water vapor and ozone to the unusual timing of El Niño and QBO disruption in 2015–2016" *by* Mohamadou Diallo et al.

Anonymous Referee #2

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Review of "Response of stratospheric water vapor and ozone to the unusual timing of El Nino and QBO disruption in 2015-2016" by Diallo et al.

This study examines the combined impact of the 2015/2016 QBO disruption and El Nino event on lower stratospheric ozone and water vapour concentrations using satellite data. It is clearly written and presented and I believe suitable for publication in ACP pending minor revisions.

Major comments:

1. In Sec. 2, two previous studies examining ENSO and QBO effects on stratospheric water vapour are cited, Avery et al. 2017 and Tweedy et al. 2017, which came to

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Discussion paper

contrasting conclusions re. the combined roles of ENSO and the QBO. This study has the same goal, and reaches a conclusion that seems closer to Tweedy et al. 2017 (that the QBO had a dominant effect on water vapour following the QBO disruption). But it isn't clearly described how the current study differs in its approach from these previous two. Is it the use of MLS data? The multiple regression approach? Please clarify what is distinct about this study and how it builds on the previous ones. Some more detailed discussion of how the results compare to the previous studies might also be appropriate in the Discussion section.

2. In Sec. 3, random & systematic uncertainties are quoted for the MLS data that seem similar in size to the regression signals reported here (note also the p4, line 5 comment below). It is also noted (p4, line 3) that unrealistic values in the low-latitude UTLS were a problem in previous versions of the MLS data, which sounds worrying since that is the main region of focus in this study. I'm not sure how to compare the reported uncertainties to the regression values. Are these random uncertainties that are applicable to single measurements, such that the regression would effectively beat down the noise? Are there systematic offsets (biases)? More discussion of what these values represent and how they could affect the results would be useful here. It's good that a number of references for data quality are provided (p4, lines 7-9), but a concise explanation of why the regression results in this paper should be believable should also be provided here.

3. In Sec. 4 (p5, line 30) it says that the differencing of residuals gives results similar to direct calculations. In that case, why not just do the direct calculation? Perhaps the lead author's previous work explains this, but a concise explanation should be given here. If there's an advantage in doing it this way, what is it?

Comments by page & line number:

p2, 1: "This moistening" - are you referring to methane oxidation?

p2, 22: On p9, line ∼15, you say that easterly shear in the tropical lower stratosphere

speeds up the shallow branch of the Brewer-Dobson circulation. But here you say that westerly shear is associated with enhanced poleward transport. These seem to contradict each other, please clarify.

p2, 26: "A major" –> "Another major"

p2, 30: Here it says that El Nino cools the lower stratosphere, but you go on to say (p3, line 16) that El Nino warms the tropopause. Please add some additional comments here to explain the distinction between the tropopause response to El Nino and lower stratospheric response. As shown in Mitchell et al 2014 (Signatures of naturally induced variability in the atmosphere using multiple reanalysis datasets), Fig 15b shows tropospheric warming and stratospheric cooling with a node near the tropopause. How robust is the tropopause response to ENSO? If tropopause warming is a distinct regional feature of the ENSO response (you go one to discuss the distinction between zonal-mean and regional responses to ENSO), this would be a good place to introduce and describe those differences.

p3, 25: "contains" –> "describes"

p4, 5: Why are O3 uncertainties give as percentages but H2O uncertainties are given in ppmv? Since the figures show O3 and H2O changes in percent, percentages for all these uncertainties would be useful.

p4, 21: Unclear what "properly" means here, suggest delete it.

p5, 5: What does "sorted out" refer to? Please be more specific.

p5, 11: Not sure what "breaking the easterly-westerly phase asymmetry" refers to in this context.

p5, 21: Insert "as expected" before "due to", since the upwelling is not actually observed.

p5, 31: "basis functions" - do you mean the predictor time series (indices)? Please

clarify what is meant here.

p6, 8: "controlling" –> "warming"

p6, 14: Insert "as expected to be" before "due to" (for same reason as the p5, line 21 comment).

p7, 9-10: If the H2O anomalies are delayed, how can they be in phase with the O3 anomalies? Perhaps say "roughly in phase", if this is what you mean.

p7, 13: "by enhancing" –> "consistent with"

p9, p4-5: Based on eyeballing Fig 4, I think I agree. But could this conclusion be made more quantitative, e.g. by saying what is the fraction of variance of the deseasonalized time series that's captured by QBO and ENSO? Or plotting the residual of the full regression in the same style as Fig 4? (Perhaps to include as supplemental so as not to clutter Fig 4.)

p9, 7: "controlled by" –> "dominated by" seems more appropriate to me since the responses here are linear by definition (because multiple linear regression has been used to diagnose them).

p9, 9: "predominated" –> "dominated"

p10, 8: Not clear what "robust" means in this context; suggest delete it.

p10, 18: "led positive" –> "led to positive"

p10, 29: "turn out to be" –> "are"

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